III. *On the Years and Cycles used by the Ancient Egyptians. By the* Rev. Edward Hincks, D.D. (Communicated by the President.)*

Read 9th April, 1838.

MUCH has been written on the subject of the Egyptian year; but I apprehend that no correct information respecting it is any where to be met with. It has been generally stated, that it *originally* consisted of 360 days; and that at some epoch, on which authors are not agreed, five additional days were annexed to it, in order to approximate more closely to the length of a solar revolution.

In opposition to this received opinion, I venture to lay before the Academy the five following propositions, which I hope to be able to establish in succession.

1st. In the early part of the eighteenth century before the Christian era, there occurred a marked chronological epoch in Egypt.

2nd. *Before* this epoch, the Egyptians used a year, of which the commencement took place at a fixed astronomical season, and the average length of which was, consequently, that of the tropical year; while *after* this epoch they used the wandering year of 365 days.

3rd. Between this chronological epoch and the year of our Lord 34, there elapsed *six* cycles, of some sort or other.

4th. The nature of these cycles was such, that in one of them the astronomical phenomenon, which had marked the commencement of the old fixed year, travelled forward through a fifth part of the wandering year, or seventy-three days; and, consequently, that in five such cycles that phenomenon returned again

* To prevent the possibility of misconception, it seems proper to state, that this paper was not sent to the President until finished; and, of course, that he is not responsible for the accuracy of any statement, result, or reasoning that it contains; having merely had the kindness to communicate to the Academy what was transmitted to him for that purpose.

to the commencement of the wandering year, having taken place on every day of it.

5th. The length of each of the smaller cycles was 300 years. Consequently, the epoch when the wandering year was introduced was 1767 B. C.; and the first day of the first year was the 8th November, 1767, according to the proleptic Julian reckoning.

Of the truth of the two first of these propositions I have long been convinced. The three last are the result of an investigation, which was suggested to me by a reference to a passage in Tacitus, which I noticed in an article on the Pyramids in Fraser's Magazine for November, 1837. On examining the passage referred to, I felt convinced that the ingenious author of the article had drawn an incorrect inference from it; and, endeavouring to ascertain what information it really conveyed, I became satisfied of the truth of the third of the above propositions. From this I soon passed to the fourth and fifth, the latter of which, being the grand result, to which the rest are subsidiary, I have since been able to confirm by independent arguments.

I. The first proposition I by no means offer as a *new* one. It is an obvious consequence of the discoveries of the late lamented Champollion, respecting the hieroglyphical notation of the year; and it must be at once acquiesced in by all who are acquainted with those discoveries. I shall, however, say a few words in explanation and support of it.

It was demonstrated by Champollion that the Egyptians divided their year, exclusive of the Epagomenæ, into three seasons; and that they denominated them hieroglyphically the first, second, third, and fourth months of these three seasons. He interpreted the characters which stood for the three seasons to mean, respectively, vegetation, ingathering, and inundation. Whatever doubt there may be as to the correctness of the two former interpretations, there can be none as to the last. It is beyond all question that the hieroglyphic names for the four last months of the year are the first, second, third, and fourth months of the inundation. Now, as the Egyptian year of 365 days was in its nature a wandering one, and as any given day of it would in course of time pass through all the seasons of the solar year, it follows that the seasons of the wandering year would *sometimes* coincide with those seasons of the fixed year, of which they bore the names. These epochs of coincidence between the wandering year and

a supposed fixed year are easily discoverable; and of this kind is the epoch, of which I speak in this proposition. We know that the inundation commenced about the summer solstice. In order, then, to discover the years in which the required coincidence took place, we have only to ascertain the years in which the summer solstice fell on the 241st day of the Egyptian year. *Perfect* accuracy is, of course, not to be expected. The solstice would, in fact, occur on this day for four successive years, and the fluctuations in the seasons arising from meteorological causes, as well as the difficulty of making an exact observation of the first rise of the Nile, would leave room for an error of perhaps twenty years on either side of the year determined by calculation. I mean to say that in any of these forty years the seasons of the wandering year could not be *observed* to differ from those seasons of which they bore the names.

Now, I find by calculation, that on the 241st day of the Egyptian year, which commenced on the 30th October, 272 B. C., that is to say, on the 27th June, 271 B. C., the solstice occurred shortly after the Egyptian noon. We may then reckon from 291 B. C. to 251 B. C. to be a period of apparent coincidence between the seasons of the wandering year and those seasons of the true year after which they were called; and the epoch of coincidence, *as observed by the Egyptians*, must have fallen between these extremes, though it would not necessarily fall in the middle year 271, which is pointed out by astronomical calculation. Going back through all the seasons, I find again that on the 241st day of the Egyptian wandering year, proleptic or actual, which would or did begin on the 11th November, 1780 B. C., that is, on the 9th July, 1779 B. C. the solstice occurred about Egyptian noon. This gives for the period of apparent coincidence 1800 B. C. to 1760 B. C.; and I am justified in saying, that within these limits a remarkable chronological epoch must have occurred.

II. Thus far, as I have already intimated, I have advanced nothing but what will be generally admitted by those who have given their attention to hieroglyphical discoveries. I now, however, bring forward a proposition, in maintaining which I believe I stand alone, namely, that up to this chronological epoch, which I have last mentioned, the Egyptians used a year, of which the average length was that of the tropical one, its commencement being marked by a phenomenon, depending on the sun's annual revolution.

It is, in the first place, manifest, that the hieroglyphical notation of the

months must have been adopted at a time, when the seasons of the actual year, of whatever sort that may have been, coincided with the seasons represented by their names. This is a proposition, which cannot, I think, be questioned; and it furnishes us with a criterion, by which we may at once reject many suppositions respecting the origin of the wandering year as *impossible*. The only hypotheses which will stand this test, besides that which I have stated above, are the following:—1. That the year of 365 days succeeded a year of 360 days at the chronological epoch of the eighteenth century, the hieroglyphical notation of the months being then first used; 2. That at this chronological epoch the hieroglyphical notation of the months was introduced; the year of 365 days having been previously in use, but the months having been otherwise noted; 3. That the hieroglyphical notation was first used for a year of 360 days; 4. That the year of 365 days, with its hieroglyphical notation, was introduced at a chronological epoch similar to that of the eighteenth century before our era, but occurring in the thirty-third century before it. In deciding which of these several suppositions is the correct one, we have to consider their intrinsic probabilities, and also the testimony of ancient authors, so far as this has been given in favor of, or in opposition to, any; and it will be well for me to state, in the first instance, that the argument that I am about to use is a disjunctive syllogism. I hope to be able to show, that all the suppositions, which I have above enumerated as *possible*, in reference to the criterion first laid down, except only that which I have stated to be my own, are either highly improbable—I may even say absurd, or are altogether opposed to the testimony of antiquity. On the other hand, I maintain that my own hypothesis is both intrinsically probable, and conformable to the testimony of such ancient authors as have alluded to the subject.

1. The first of the four hypotheses, which compete with my own, must, I conceive, be rejected on account of the extreme improbability that the Egyptians should have continued to use a year of 360 days so late as the beginning of the eighteenth century before our era. A great number of centuries must then have elapsed since the peopling of Egypt, even according to the lowest biblical chronology; the inhabitants must have had considerable intercourse with neighbouring countries; and we know that they had attained to no small degree of civilization. Can we then suppose with any reason, that, up to this late period, they should know no better than to measure their time by a year of 360 days;—

a year, which, while it had no relation to the phases of the moon, would have represented so inaccurately the course of the sun, that its commencement would pass in thirty-five years from midwinter to midsummer, and in seventy would go through the entire round of the seasons? That such a year should have been tolerated *for centuries* in *any* country, and more especially in Egypt, where the striking annual phenomenon of the inundation must have attracted the attention of every individual, is, in my judgment, a supposition which cannot be entertained for a moment. It is alleged, however, by its advocates, that the testimony of antiquity is in its favour. I readily admit that there has been a very general consent among *modern* authors, as to the supposed fact of a year of 360 days having been in use before the year of 365 days; but I deny that any author, who deserves to be called *ancient*, has given countenance to such an opinion. Plutarch, indeed, records a fable, that "the Sun, having discovered the infidelity of his wife Rhea, prevented her by a curse from bringing forth her offspring on any of the 360 days of the year; but that Hermes, playing at dice with the Moon, won five additional days, on which Osiris and his brothers and sisters were born." Such is the only *ancient* authority in existence for a year of 360 days having *ever* been in use; and it is evident that this authority, by throwing back the *disuse* of that year to the mythological epoch of the birth of Osiris, does in fact negative the supposition that a year of 360 days was ever used in the times of real history. There was, however, in the eighth century after the Christian era, a monk of the name of Georgius, (usually called, for distinction, Syncellus,) who compiled a Chronography, in which he has preserved some valuable fragments of the works of ancient authors that are lost. This writer is usually appealed to as an authority for the existence of a year of 360 days; and he certainly has asserted its existence; but then he has not asserted it on the authority of any more ancient writer, and this makes all the difference in the world. If a statement to this effect had occurred in a quotation made by Georgius from Manetho, or any ancient author that he named, that statement would have weight, arising from the antiquity or character of that author. In the present instance, however, the statement is that of Georgius himself; it is the mere expression of the opinion of a writer of uncommonly weak judgment, who lived so late as the eighth century; and it is consequently altogether worthless. I insist the more on this point, because I have seen this statement of

Georgius quoted as a statement of Manetho. Georgius gives, indeed, copious extracts from Manetho, as transmitted both by Africanus and by Eusebius; and in these extracts he mentions several facts respecting different Egyptian kings; but the passage in which he speaks of the year does not occur in any of these extracts. It is to be found (page 123 C. D. Paris edition) in the Catalogue of Egyptian Kings, which he gives *as his own*;—a catalogue, which is universally admitted to be full of the grossest errors, so as to be utterly unworthy of notice. In that catalogue he mentions a king, whom he calls Asseth; he places him immediately before Tethmosis or Amosis, and he elsewhere says that he was the father of Tethmosis. No such king is mentioned either by Africanus or Eusebius; and Josephus calls the father of Tethmosis Alisphragmuthosis. There was, therefore, some reason for Scaliger to wonder, "whence Georgius fished up this king Asseth." Josephus, however, mentions a king Assis, the last of the shepherd sovereigns; and this appears to have been the Asseth of Georgius. It is true, that, according to Manetho, as preserved by Josephus, the reign of Assis terminated 251 years before that of Tethmosis began; and that Assis was one of the Shepherd conquerors, while Tethmosis was the native prince who expelled them. These would be no objections in the eyes of Georgius. It would be quite in accordance with his peculiar method of cataloguing kings to place these two sovereigns in the relation of father and son! After mentioning Asseth, Georgius makes the following remark:—"He added the five additional days of the years; and in his time, as they say, the Egyptian year was appointed to consist of 365 days, when it before this was composed of only 360." Here we have the statement, which later writers have so generally acquiesced in; and we have it *repeated* in the same sentence, apparently for greater emphasis; but it still rests on the authority of Georgius only; and I can by no means esteem the authority of a blundering writer in the eighth century, as sufficient to establish a fact, which is intrinsically so improbable. But, it will be objected, would Georgius have been likely to invent such a statement? Must he not have had some foundation for it in some ancient writing now lost? I grant that it is improbable that he fabricated such a statement *without foundation*; but I think there is every probability that he misunderstood the statement of the unknown author which served him as a foundation. In the *double* statement of Georgius we may, I think, discern the original text of the unknown writer and the glosses

of his ignorant copyist. The clause in which he inserts the words of quotation, (ὥς φασι,) I take to have been copied, and to contain a statement which is probably true; the preceding and following clauses I believe to be Georgius's own, and to contain his blundering gloss on the original statement, and his re-assertion of it when perverted by that gloss. I conceive that the unnamed author simply made the following statement. "In his time"—it is uncertain whether he is speaking of Assis, the shepherd, or of the father of Tethmosis; but it is certain that these were not identical, as Georgius imagined;—"in his time the Egyptian year was appointed to consist of 365 days." The chronological epoch of the eighteenth century before Christ might very well occur in the reign of Assis, the shepherd, if the chronological system of Mr. Cullimore be correct; or in that of the predecessor of Amos, the founder of the eighteenth dynasty, if Champollion and Rosellini be in the right. Now, the author of this statement may have intended to point out the reign in which the wandering year of 365 days succeeded the old fixed year. But Georgius, having heard or read the statement of Plutarch, already referred to, that once on a time the year had only 360 days, explained what his author had said of the abandonment of the old fixed year, in reference to a supposed abandonment of the imaginary year of 360 days; he added the words, "which before this was composed of only 360;" and, to make his meaning still clearer, he put the entire into other words, "he added the five additional days of the years." This may, or may not, be the source of the erroneous statement of Georgius; but that the statement is erroneous, I can entertain no doubt whatsoever. The continuance of the use of a year of 360 days to so late a date as about 1780 B. C. is far too improbable to be admitted on the testimony of a writer, so recent, and of such weak judgment, as Georgius Syncellus.

2. But it may be asked, might not a year of 365 days have been substituted for one of 360, at an earlier period than 1780 B. C., when the Egyptians were less civilized, and before they had intercourse with other nations? To a believer in the divine record respecting the peopling of Egypt, it might be sufficient to reply, that no such period of want of civilization and isolation from other nations, as this question presumes the existence of, can be supposed. When Ham and his descendants settled in Egypt, they came there fully acquainted with the knowledge that had been acquired before the flood; and we cannot doubt that the

true length of the year was among the facts known. It is difficult to suppose that the excess of the solar year over 365 days should not have been known and estimated; but, as to its excess over 360 days being familiar to the first settlers, there cannot be a question. How then could it ever have occurred to them to limit the length of the year to 360 days? Even the unbeliever in revelation must see the absurdity of a year of 360 days having continued in use to so late a date as 1780 B. C.; but to the believer in the Holy Scriptures there is the same absurdity in the supposition that such a year ever existed in Egypt at all.

I write not, however, for believers in the Scriptures exclusively; and I will, therefore, without reference to their authority, reply to the question which I have supposed to be asked; and will show that a year of 365 days *could not* have been substituted for one of 360 days in the ages preceding 1780 B. C. any more than at that epoch.

The year 2782 B. C. has been fixed upon by some chronologers as that in which the year of 365 days succeeded that of 360. Those who maintain that opinion, or any similar one, will have to account for the hieroglyphical notation of the months on the different monuments. That notation could not have been introduced at the time when the year of 365 days was introduced, or at any subsequent epoch before about 1780 B. C., because until this last mentioned period the physical characters of the actual year could never have corresponded to the physical characters expressed by the notation. It remains then, that the notation must have been first introduced about 1780, *the form of the year remaining unaltered;* or that the notation must have been introduced previously to the change in the form of the year. The absurdity of the former supposition is shown in this manner. Assuming it to be the case, it must have been well known, at the time their denominations were given to the months, that they would become inapplicable to them in the course of a few years. There must have been many living, who would be able to testify, that, though the inundation commenced then at the beginning of the ninth month, it had commenced in their youthful days before the middle of the eighth, and they had heard from their fathers of its commencing in the seventh. There was no change in the form of the year cotemporary with the adoption of this notation of the months and seasons, which might give rise to the belief that *hereafter* the seasons would continue correctly denominated. The year must have been generally recognized

as being in its nature a wandering one,—as having been so, and as still to continue so; and, this being the case, it seems hard to conceive how any legislator would think of giving names to the seasons of the wandering year, grounded on their coincidence with the seasons of a fixed year; and it is still harder to conceive how the names given by such a legislator should have supplanted those previously in use, and become the only ones, by which time was hieroglyphically described for above 2000 years!

3. The next supposition which I have to refute is, that these hieroglyphical names were originally given to the months of a year of 360 days. In the first draught of this paper I had not thought it necessary to notice an hypothesis, which appeared so irrational as this; but I have since learned that M. Biot has adopted it; and respect to his talents induces me to notice his work. There does not appear to be any argument brought forward by M. Biot in support of this opinion. He assumes, as two incontrovertible propositions, that the Egyptians originally used a year of 360 days, and that their calendar was originally adapted to such a year; and all his ingenuity is employed in inventing for them cycles, which, if they had known and used them, would have obviated, in some measure, the inconveniences of the system which he ascribes to them. Now I have already shown, that there is no ancient testimony in support of the opinion that the Egyptians used a year of 360 days; and as for the form of the calendar being an evidence of it, M. Biot might have recollected that the *Sanscullotides* were an original and essential part of the year, which his countrymen adopted at the Revolution, and which was precisely similar in its construction to the Egyptian year. There is not a shadow of evidence opposed to the opinion, that the "five celestial days" were, in like manner, an original and essential part of the hieroglyphic calendar. M. Biot has stated, that "in the first ages of a nascent civilization a year of 360 days, divided as in Egypt, would express the annual series of operations of agriculture with a fidelity which should have been *long* sufficient (qui a dû *long-temps* suffire)." In reply to this, I will only observe, that, in the short space of thirteen years, during which the French revolutionary calendar continued in use, if the sanscullotides had been omitted, the commencement of the year, and of course that of each of the months, would have deviated sixty-eight days from its original place; the vintage would scarcely be over when *Nivôse* would commence, and the snow might be still on the ground in the

beginning of *Germinal*. I can conceive it to be possible that the French Revolutionists might have adopted a year of this form; but, had they done so, I cannot think they would have given *names* to the months, *expressive of their physical characters*;—not even though they had a Delalande to point out to them that "in sixty-nine or seventy years, or more accurately three times in 209 years, the months would return to their *normal* places." It is surprising to me that M. Biot did not perceive that the establishment of any cyclical relation between the year of 360 days and the tropical year supposes a knowledge of the length of the latter. Had the use of the tropical year preceded that of the year of 360 days, such a relation might have existed; but this is not M. Biot's opinion. He attributes the use of the year of 360 days to ignorance. It originated, according to him, in "the first ages of nascent civilization," ages of which I deny the existence in Egypt; and was abandoned when it was discovered that a year of 365 days would more accurately exhibit the succession of the seasons. Under such circumstances, how could a cycle, such as M. Biot has imagined, have been employed? The demi-savages, whom he supposes to have then inhabited the valley of the Nile, had not *him* to reveal it to them. In truth, the parts of M. Biot's memoir, in which he treats of the cyclical relations of this year of 360 days, are but a specimen of ingenious trifling. Till he had brought forward some proof that it existed at all, and, if so, that it existed cotemporaneously with the hieroglyphic notation of the months, he need not have troubled himself to show that in 209 years such a wandering year would have its seasons three times in coincidence with their primitive places; while in 487 years the dogstar would seven times rise heliacally at its commencement.

4. There is a fourth way of accounting for the hieroglyphic notation of the months, as it exists on the monuments. The notation might have been introduced, with or without a change in the form of the year, at the chronological epoch, similar to that of the eighteenth century before our era, which might have occurred in the thirty-third century before it. To this I reply, that the epoch in question is prior to the most remote of the eras which biblical chronologers have assigned for the deluge. This simple statement ought to be conclusive against the hypothesis. But, as this paper may fall into the hands of some who undervalue this consideration, I will lay before them some others.

I might appeal to the fact, that no dated inscriptions have been discovered,

which even Champollion, who was disposed to give the highest possible antiquity to them all, could refer to an earlier age than the twenty-first century before our era. Is it probable that the hieroglyphic calendar should have been in use for twelve centuries before that time, and that no monumental records of its existence should remain? But I will rather apply myself to expose the fallacy of the grand argument, by which they, who throw back the origin of the hieroglyphic calendar to 3285 B. C., or before it, pretend to establish their system.

This argument may be briefly stated as follows. There is reason to think, from certain passages in ancient authors, that the summer solstice and the heliacal rising of Sirius coincided at the time when the hieroglyphic calendar was constructed. M. Biot alleges that this coincidence took place in the year 3285 B. C.; and, though he admits that it would continue *sensibly*, or within the limits of errors of observation, for 500 years on either side of this epoch, that is, from 3785 to 2785, he seems to think we are tied down to the middle date by the consideration that *then only* the two coincident phenomena would occur at the beginning of the ninth month of the wandering year. Now I admit that it is *probable*, though it is by no means certain, that there was a *sensible* coincidence between the summer solstice, the heliacal rising of Sirius, and the 241st day of the Egyptian year, when the hieroglyphic notation was introduced; but I say that this coincidence might have occurred more than 1000 years after the epoch, which M. Biot has assigned for it, *and subsequent to the biblical epoch of the colonization of Egypt.*

In order to prove this, I chiefly insist on the points, that what is called the heliacal rising of a star depends on two uncertain elements, namely, the latitude of the place of observation, and the depression of the sun below the horizon at the time of the star's rising, which is barely sufficient to allow that star to be seen;* that M. Biot has assumed greater values for both these elements than he

* In order to determine the heliacal rising of a star, spherical trigonometry furnishes us with the following formulas: α being the latitude of the place of observation; γ the depression of the sun below the horizon necessary for the star's being seen at its rising; λ being the declination, and μ the right ascension of the star, and ω being the obliquity of the ecliptic; we have, the latitude being north, and the declination of Sirius south;

$$\tan.\alpha.\tan.\lambda = \sin.\gamma;$$

was in fairness entitled to do; and that, if he had not done so, the epoch of coincidence between the heliacal rising of Sirius and the solstice would have resulted much later than he makes it. It is, however, not unworthy of being noticed, that the *rigorous* coincidence, which he alleges to have taken place in the year 3285 B. C., and on which he appears to lay so much stress, did not really, *even on his own hypothesis*, take place in that year. The following are the solstitial dates in that year and in the two preceding and two following years, given proleptically, both according to the Julian computation and to that of the Egyptian wandering year. That for 3285 is taken from M. Biot's memoir, adding 1.93 hour for the difference between Parisian and Memphitic time; the others are deduced from this by addition and subtraction of 365 days 5.82 hours, which was in that age about the average interval between successive summer solstices.

Years B. C.			Hours after Memphitic Midnight.
3287	July 21st	Pachon 1st	10.92
3286	—— 21st	——— 1st	16.74
3285	—— 20th	——— 1st	22.56
3284	—— 21st	——— 2nd	4.38
3283	—— 21st	——— 2nd	10.20

$\mu + \nu$ being the arc of the equator between the first point of Aries and the horizon, at the time when Sirius is rising;

$$\frac{\cos.\omega.\cos.(\mu+\nu) - \sin.\omega.\tan.\alpha}{\sin.(\mu+\nu)} = \cot.\theta_0;$$

θ_0 being the arc of the ecliptic between the first point of Aries and the horizon;

$$\frac{\sin.\gamma.\sin.\theta_0}{\cos.\alpha.\sin.(\mu+\nu)} = \sin(\theta_\gamma - \theta_0;)$$

θ_γ being the arc of the ecliptic between the first point of Aries and a parallel of altitude, the vertical depression of which below the horizon is γ.

The morning, on which the sun's longitude *first exceeds* θ_γ, is the morning on which Sirius is said to *rise heliacally*.

Now θ_γ is, as we have seen, a function of five quantities, α, γ, λ, μ, and ω. The three last are determined, the time being given; but they vary with the time of observation. The two former are independent of the time; but α is different for different places of observation, and γ for different stars. The more brilliant the star, and the more remote its place of rising from the part of the horizon which is over the sun, the less will γ be.

It appears from inspection of this table, in the first place, that the year 3285 B. C. ought not to have been selected as the year of coincidence between the solstice and the first day of Pachon, but rather 3287 or 3286. It appears also that about this period the summer solstice fell regularly on the 21st of July of the proleptic Julian year, and was only beginning to fall on the 20th in leap years. M. Biot, instead of *directly* determining the coincidence of the solstice and the heliacal rising of Sirius, which would be a purely mathematical problem, independent of any artificial divisions of time,* uses the 20th of July of the

* From the definition of heliacal rising, the sun's longitude at the time of the star's rising must exceed θ_γ, but by a less quantity than the space which it passes over in a day. Its average excess over it in a period of four years may be estimated at half this space, say 29′. 30″. Consequently, when the heliacal rising coincides with the solstice, θ_γ must be equal to the difference between 90° and this last-mentioned quantity, or to 89°. 30′. 30″. If λ, μ, and ω be calculated for any epoch, their values for other years may be expressed by series of the form $A_0 + A_1 t + A_2 t^2$, &c., t being the number of years after the epoch; and by the formulas of the preceding note, $\mu + \nu$, θ_0, and, ultimately, θ_γ may be expressed in similar series. The value of t, which will satisfy the equation $\theta_\gamma = 89°. 30'. 30''$, will give the precise number of years after the assumed epoch, at which the required coincidence took place. In order to simplify the calculation, the epoch for which λ, μ, and ω are calculated should not be far removed from the epoch of coincidence. In that case, we may confine ourselves to the terms in the above series which are independent of t, or contain only its first power. These terms will at any rate give a first approximation; and we may then calculate the values of λ, μ, and ω for the year so found as a new epoch. The great practical difficulty arises from the uncertainty which there is as to the proper motion of Sirius, and as to the precession, and change of the obliquity of the ecliptic. According to the best data that I have been able to procure, (namely, the values of the precession and obliquity given by M. Biot from Laplace's formulas, and the proper motion given in the catalogue of the stars in the Encyclopædia Metropolitana,) I make the right ascension of Sirius in 3285 B. C. to have been 43°. 47′. 15″, and his declination 23°. 37′. 45″. The former was increasing at the rate of 38″,43 a year, the latter diminishing at the rate of 13″,64 a year. The obliquity was 24°. 6′. 30″, and its diminution annually 0″,33. Substituting these values for λ, μ, and ω in the formulas of the preceding note, and making $\alpha = 30°$ and $\gamma = 11°$ (the values assumed by M. Biot) I find

$$\mu + \nu = 58°. 25'. 6'' + 28'',45\, t;$$
$$\theta_0 = 74°. 7'. 43'' + 28'',36\, t;$$
$$\theta_\gamma = 88°. 32'. 1'' + 25'',95\, t.$$

In these expressions, the rates of increase are much more to be depended on for accuracy than the values at the epoch.

From the equation

Julian year as a middle term. The heliacal rising, he alleges, *always* occurred on that day; the solstice occurred on that day in 3285 B. C. In that year, therefore, they rigorously coincided. This appears plausible; but I would ask, in the first place, did the heliacal rising of Sirius occur *in every year* on the 20th of July? Would not the intercalation, which threw the solstice from the 21st to the 20th, have thrown the heliacal rising from the 20th to the 19th? We are, perhaps, not in a state to answer these questions, either affirmatively or negatively, from our ignorance of the precise amount of the change that the position of Sirius has undergone, in the long interval of 5000 years, from its own proper motion and from the precession of the equinoxes.* But, secondly, admitting that the rising of Sirius on the 20th July was the heliacal rising, was this the rising that coincided with the solstice? Is it not obvious, on the contrary, that the rising in the early morning of the 21st was the rising which coincided with an event that occurred at half-past ten in the preceding night? There was then no *real* coincidence between the heliacal rising of Sirius and the solstice in 3285 B. C. M. Biot must admit that there was none in the preceding or following years; and that which, he endeavours to show, took place in this year is only a colourable one, depending on the arbitrary commencement of the

$$88°.32'.1'' + 25'', 95\,t = \theta_\gamma = 89°.30'.30''$$

we have

$$25'', 95\,t = 58'.29'';$$
$$t = 135.$$

If then the values of λ, μ, and ω that I have used be correct, the coincidence occurred in 3150 B. C.; or a few years earlier, as the coefficient of t^2 was positive, and the average rate of increase of θ_γ in 135 years was on this account somewhat greater than the rate at the commencement of the period. The coincidence would continue for as many years before and after this date as θ_γ would take to increase 29'.30'', or whatever was the exact value of the sun's motion in longitude for half a day. That is to say, it would continue from about 3215 B. C. to about 3085 B. C.

* If the value of θ_γ for 3285 B. C. be correct, the sun would have attained that longitude about thirty-six hours before the solstice; that is, about half-past ten in the morning of the 19th July. He would consequently have been some ten or eleven minutes less than 11° below the horizon at the time when Sirius rose. I should think this was within the limits of probable error in the computation. The occurrence of the heliacal rising of Sirius on the same day of the Julian calendar, which was the case for a great number of centuries, is owing to the excess of the mean Julian year, 365.25 days, over the tropical year, being very nearly equal to the time that the sun would take to pass over the annual increase of θ_γ.

artificial day. It seems absurd to lay any stress on a coincidence occurring rigorously in any specified year, when the phenomena which coincide approach one another at the very slow rate of about eleven minutes a year, and when they would, of course, continue to coincide for about 130 years. But, as M. Biot has insisted a good deal on this coincidence having rigorously taken place in 3285 B. C., it seems proper to show that his statement to that effect is unfounded. The 130 years of coincidence did not begin till about seventy years after this epoch.

The error which M. Biot has committed in this matter is, however, comparatively of little importance. I now proceed to show that the suppositions which he has made in his calculation respecting the latitude and the arc of depression are altogether unwarranted. The former he assumes to be 30°, and the latter 11°. Now I contend that both of these values have been taken unwarrantably great; and the extent to which this vitiates the calculation may be judged from the following statement; a diminution either of the arc of depression or of the latitude by one degree will bring down the epoch of coincidence above 150 years.* A person aware of these facts might easily exhibit an apparent coincidence between the phenomena in question on any assigned year of perhaps 1500. He has only in the first instance to choose a suitable arc of depression; then to calculate under what parallel of latitude, *assuming this arc*, the star would rise heliacally at the solstice of the assigned year; and lastly, to invent plausible reasons for using that arc, and for placing his observer under that parallel. It appears to me that this is just the course which M. Biot has pursued. His curious reason for choosing the latitude of 30° seems to prove it. He takes the latitude of Memphis; "because for epochs so ancient we cannot place the centre of religion *in the very lowest parts of Egypt*." This *one-sided* reason shows plainly what was passing in his mind.

* Using the values of λ, μ, and ω for 3285 B. C., as laid down in a former note, I find that a substitution of 10° for γ, in place of 11°, diminishes θ_γ by 1°. 19′. 11″; a substitution of 29° for α, in place of 30°, diminishes θ_γ by nearly the same quantity, namely, by 1°. 20′. 11″. To compensate for this diminution, θ_γ must be augmented by the terms depending on the time; and, allowing for the increase which the coefficient of t would undergo, as well as for the necessary introduction of the term containing t^2, it will appear that either of these substitutions must bring down the epoch of coincidence considerably above 150 years.

He would gladly have reduced the arc of depression, which he must have seen to be too great, by a couple of degrees; but had he done so, he must, by way of counterpoise, have added as many degrees to the latitude, and thus descended to the extremity of Egypt, which he could not venture to do. But why place "the centre of religion" so low down the Nile as Memphis? For doing so he has not assigned the shadow of a reason. If we are to seek the cradle of the Egyptian religion, and view the heavens from thence, tradition refers us not to Memphis or Heliopolis, but to Philæ, the reputed burial place of Osiris, and the most sacred spot in the country. I will not be so unreasonable, however, as to take M. Biot to the most southern part of Egypt. I propose that we divide the interval, and take Thebes for our observatory. It was the most ancient capital of Egypt, as all are agreed. The latitude of Thebes is 25°. 45′, four degrees and a quarter south of M. Biot's parallel, answering to about 600 years, by which I contend that I am entitled to bring down the epoch of coincidence between the solstice and the heliacal rising of Sirius. I have, in fact, calculated the solstice and the *Theban* heliacal rising for the year 2550 B. C., using, as M. Biot has done, 11° for the arc of depression; and I find that they took place on the same day, in that and many following years.*

But in making this calculation I erred by taking so large an arc of depression as 11°. M. Biot's reason for taking it is, that it was the value of that arc adopted by Ptolemy. I grant it; but this seems to me a sufficient reason for rejecting it as excessive. Ptolemy gives it as the arc of depression of the sun below the horizon, which would allow *a star* to be visible at its rising. He uses it for all stars alike, taking no notice of the inequality in this arc, which their unequal brilliancy and the unequal distance of their places of rising from the part of the horizon over the sun would require. Now, I argue, that, if 11° be the proper

* In 2550 B. C. I find, using the same data as before, $\lambda = 20°.\ 51'.\ 40''$; $\mu = 52°.\ 16'.\ 0''$; $\omega = 24°.\ 2'.\ 8''$; whence we have, for the latitude of 30°, $\theta_0 = 80°.\ 31'.\ 48''$; $\theta_{11} = 94°.\ 24'.\ 25''$; but for the latitude of Thebes, (25°. 45′) $\theta_0 = 76°.\ 6'.\ 12''$; $\theta_{11} = 89°.\ 27'.\ 52''$. This year, then, was about the middle one of those in which the solstice coincided with the heliacal rising of Sirius at Thebes; that is to say, on M. Biot's assumption that the arc of depression should be taken so great as 11°. I cannot but think that 9° or 9°. 30′ would be fully sufficient. Now I find that in 2550 B. C., at Thebes, $\theta_9 = 87°.\ 1'.\ 29''$; $\theta_{9,5} = 87°.\ 38'.\ 2''$. Subtracting these quantities from 89°. 30′. 30″, and dividing the remainders by 27″, we have in the former case 330 years, and in the latter case 250 years, as the intervals between 2550 B. C. and the mean epochs of coincidence.

arc of depression for a star of average brilliancy, it is quite too great for a star of such preeminent brilliancy as Sirius; especially when the distance in azimuth of its place of rising from the sun was upwards of 60°. M. Biot allows that the observation of heliacal risings admits an uncertainty of three or four days at least; but one principal cause of this uncertainty is, that the heliacal risings of bright stars must precede, while those of faint ones must follow, the times calculated on the hypothesis of their rays having a uniform power. On the ground of the possible errors of observation, he allows 500 years *before* or *after* the epoch of 3285 B. C., as limits, within which the heliacal rising would sensibly coincide with the solstice. It appears to me, that there would be little likelihood of the heliacal rising of Sirius being *later* than the time calculated on the supposition of the sun's being 11° below the horizon, but that there would be every probability of its being *earlier*. It would, therefore, I contend, be right to lower the epoch on this account by *at least half* of this admissible error of 500 years. I have further to add, that there is a different source of errors of observation, which M. Biot has overlooked, but which should evidently be taken into account. He has only considered the possibility of erroneously observing the heliacal rising of Sirius; but, surely, an error in observing the solstice is to be expected also. He seems to have forgotten, that, in the whole of this argument, what has been called the solstice is not the observed arrival of the sun at the tropic of Cancer, but the observed commencement of the inundation of the Nile. In this observation an error of four or five days might easily take place, which would correspond to 520 or 650 years in the epoch. For this admissible error of observation, and for that which may remain in observing the heliacal rising, I conceive that 600 years will be a very moderate allowance. To sum up the whole of this argument, I take from M. Biot's epoch 135 years, in which I conceive that he has erred as to the epoch of coincidence between the solstice and the heliacal rising of Sirius, even on his own hypothesis as to latitude and depression. I take 600 years more, in which he has erred by taking the heliacal rising at Memphis in place of that at Thebes; and 250 years more, which he should have allowed for the superior brilliancy of Sirius to the average brilliancy of the stars mentioned by Ptolemy in his Apparitions. This reduces the epoch of *accurate* coincidence between the heliacal rising of Sirius and the solstice from 3285 B. C. to 2300 B. C.; and I maintain that there would be a *sensible* coincidence, within the limits of errors of observation,

for 600 years before and after this last epoch; that is, to so late a date (at Thebes) as 1700 B. C. M. Biot, however, lays great stress on there having been a *treble* coincidence in the year 3285. Not only the heliacal rising of Sirius, *as computed by him*, and the solstice, but also the first day of the ninth month of the year, coincided in that year; and, whatever be the case as to the solstice and the heliacal rising of Sirius, he thinks that the solstice and the commencement of the ninth month could never have coincided between 3285 and 1780. Now, I admit that there is great apparent force in this reasoning, and it would be difficult to answer it on any other hypothesis as to the nature of the year than that which I am maintaining in this proposition. According to *this* hypothesis, however, *the objection does not lie.* According to it, there was *invariably* a sensible coincidence between the solstice and the beginning of the ninth month, from the first colonization of Egypt down to the beginning of the eighteenth century before Christ. And there was, consequently, by what I have just proved, a sensible coincidence between *all the three events*, not for a few years only, as M. Biot supposes, but for the whole period between the peopling of the country, and the change of the form of the year in the eighteenth century before our era.

5. I will now proceed to develope this hypothesis of my own, for which I have cleared the way by assigning special reasons why every other possible hypothesis should be rejected. Let me first, however, mention one grand objection, to which they are all in common liable. They none of them account in a satisfactory manner for the hold which the hieroglyphical notation of the seasons gained on the affections of the people. A wandering year existed in the country for 2000 years, the names of the months and seasons of which were descriptive of their physical characters at a particular epoch. If we say that the names were first given at that particular epoch, we in some measure account for their first introduction; we account for *these* names having been given rather than any other names descriptive of physical characters. All the hypotheses that we have been considering go *thus far;* but this is not enough; and they none of them go further. They do not account for names descriptive of physical characters being given to the months of a wandering year, rather than names expressing simply the order of succession, or names derived from the deities, which were supposed to preside over them. It is a remarkable fact, that names of this latter kind *existed*, and *might have been used*, but that *they never are used in*

expressing dates. The month Athur *might have been expressed* in a date by the known symbol of the goddess of that name, who presided over it; namely, a hawk within an enclosure; but it *always is expressed* by a much more complicated group of hieroglyphics, signifying "the third month of vegetation." Now, on any of the hypotheses which we have been considering, except the first, it is a strange and unaccountable circumstance, that names of this last kind (names expressive of physical character) should have been given to the months *at all;* and on any of the hypotheses, the first inclusive, it is unaccountable how they continued in use, after they were found not to represent correctly the physical characters which they professed to represent. There is, it appears to me, only one way, in which this most remarkable fact can be accounted for. The names were first applied to the months of a *fixed year;* they continued to be applied to the months of such a year, until the use of them was firmly established by custom; and, when a wandering year was substituted for the old fixed one, the deviation was gradual; there was no violent change, sufficient to overcome the force of habit, which would plead powerfully for the retention of the old names.

But, it will be asked, how can we suppose it possible that the Egyptians, if they had ever enjoyed the advantage of having a fixed year, would abandon it, and adopt the less perfect year of 365 days in place of the more perfect one? To this I reply, that the Egyptians had a different notion of what a year ought to be from what we have; and that we have no right to question their having acted in a particular manner, merely because, if *we, with our present feelings*, had been in their situation, we should have acted otherwise. I conceive that, according to Egyptian notions, the year of 365 days, as it existed in the age of the Ptolemies, and for fourteen or fifteen centuries previously, was the perfect model of what a year ought to be; that the change, which introduced it in place of the old fixed year, would be considered as a grand reformation of the calendar; and that the getting rid of the 366th day, which had previously occurred at the end of certain years, would be regarded as the getting rid of an abominable nuisance.

This is not a mere conjecture of my own; the testimony of antiquity decidedly favours this opinion. Geminus, the most ancient writer extant, who alludes to the form of the year, further than simply to describe it, says that "it possessed a great advantage in the estimation of the Egyptians, in that it sancti-

fied all the seasons equally, by bringing to them in their turn all the feasts of the year." The kings of Egypt were required to swear at their accession "that they would allow no intercalation of month or day, but would adhere to the 365 days, as their ancestors had appointed;"—a plain evidence of what I have somewhere seen stated as a fact, that some Egyptian king had attempted to restore the year to its original state by intercalating a month to make up for the days already lost, and by ordering single days to be intercalated afterwards on the old system; but that a popular insurrection had compelled him to abandon the project. The oath, it would seem, was imposed upon him at that time; and his successors were regularly required to take it. We have again a valuable testimony to the importance, which the Egyptians attributed to the preservation of the proper form of the year, in the complaint made by Iamblichus, after the fixed year had been substituted for the wandering one at the Roman conquest. "The change," he says, "has taken away all their force from the prayers of the people." If, in fact, we consider the religious appropriation of the days of the year to the different deities, we shall see the ground of this complaint. The year consisted of twelve months, and each month of thirty days. Now, these thirty days were parcelled out among the different deities, so that each had his own festival day occurring twelve times in the year. Each city, and probably each family and individual, had its peculiar days to be observed, while the remaining days in the month were passed over without notice. Besides these monthly festivals, there was a grand annual festival, observed on the five celestial days, in which all the Egyptians took a part. The honors paid to the kings, who were worshipped as gods during their lives, were arranged on this same system. We have a specimen in the decree on the Rosetta stone. The days, which were to be kept in honor of the young king, were *two in each month*, the 17th and 30th, because, as the decree states, the 30th Mesore was his birth-day, and the 17th Mechir was the day of his accession, and a yearly feast of *five days*, at the beginning of every Thoth, a feast equalling in length and immediately following the grand feast of the five celestial days. There is no reason to suppose that the honors appointed to be paid to Ptolemy Epiphanes by this decree were at all different from those which had been paid to his predecessors. Knowing, then, what these were from this valuable record, we obtain an insight into the whole system. We see the course of the monthly festivals; and we see how

the introduction of a 366th day into any year would not only leave a day in that year without any religious rites properly belonging to it, but would throw all the religious rites of subsequent years from those days to which they would be popularly regarded as pertaining of right. Enough, however, has been said on this subject, which is rather a matter of curiosity than of importance. I proceed to explain the nature of the Egyptian year, which was first used, and to which the hieroglyphic notation was originally adapted, more fully than I have yet done.

The commencement of the year was originally fixed, and continued for many centuries, at the period when the fall of the Nile allowed the first operations of agriculture to commence. This may have been ascertained in the first instance by some kind of nilometer, which would mark the time when the Nile in its descent reached some standard height. It is not necessary to suppose that the year consisted, at the first introduction of this system, of months of thirty days, with additional days in the end. The division into three seasons probably preceded the division into months; and I think there is reason to suppose that these seasons were equal; or rather that two of them, probably the first and third, contained 122 days each, while the middle one contained 121 in ordinary years, and in what we should call leap years 122. My reason for this conjecture is, that in the final result, to which my researches have conducted me, I find the solstice to have occurred on the 244th, and not on the 241st day of the year. This might have been occasioned by an inaccurate observation, i. e. a late inundation, in the year, which happened to be selected as the standard one; but it appears more probable that the three seasons were for a time as nearly as possible of the same length; and consequently that the solstice was properly placed on the 244th day of the year, that being the first day of the third of the seasons. The determination of the commencement of the year by a nilometer was objectionable, as it would not give years of the same length. A year so determined might perhaps contain 370 days, or it might contain no more than 360; but, *on an average*, it is evident that the length of such a year must have been that of the true solar or tropical year. To avoid this inconvenience, another mode of determining the first day of the year was adopted, probably at a very remote period, very little subsequent to the colonization of Egypt. This method consisted in observing the meridian shadow cast by the sun on the first day of the year. The length of that shadow was measured in *some one year* on its first day, determined either

by the nilometer, or by its being the 122nd after the solstice, and that length was thenceforward considered as the standard; and the day, in which the shadow, in its increase after the summer solstice, attained to that standard length, was accounted the first day of the new year. The years so determined would be of the same average length as before, but they could only consist of 365 and 366 days. They would arrange themselves in periods, consisting of three common and one protracted year, occasionally interrupted by periods consisting of four common and one protracted year. During the time that this system continued in use, and probably shortly after its introduction, the year was divided into twelve equal months of thirty days, and the five or six days, which it contained additional, were placed together at the end. To this form of year the hieroglyphical notation was adapted. The names given to the months in that notation expressed physical characters, which they were known to possess, and which they must continue to possess so long as the form of the year should remain as it was. In order to determine the first day of the year by the meridian shadow cast by the sun, it was necessary that there should be some object of a remarkable appearance, terminating in a point, and of a permanent nature, the shadow of which might be measured. The pyramids possessed all these characters in a remarkable degree; and I cannot doubt, that, whatever end they might be intended to answer of a sepulchral or religious nature, they were constructed externally with a view to their being used to mark the commencement of the year, and that they were actually used for this purpose. The ingenious author of the articles on the Pyramids in Fraser's Magazine, conceives that they were designed to mark the commencement of the wandering year at the period of their erection; the day of the pyramid's first casting a shadow being the day corresponding to the first day of the wandering year at the time when the pyramid was built. But in the long course of time, which must have elapsed while a pyramid was being built, the sun's altitude on the first of Thoth must have varied considerably, supposing, as this writer does, that the year was then a wandering one. Besides, the commencement of the building of a pyramid does not appear to have been an event of such importance as that its era should have been marked in this manner. I should think it much more likely that the pyramid was constructed so as that the first day of its casting a shadow at noon should be the first day of *a fixed year*; and there appears some reason to think that, in

some of the pyramids at least, this was the case. It is a remarkable fact, that the date assigned by this writer for the erection of the great pyramid of Jizeh is the identical year of the epoch, at which I have been led to place the reformation of the Egyptian calendar; he calculates that it would cast its first shadow on the first of Thoth in 1767; and that is *the very year*, to which the cycle that I have discovered conducts me from A. D. 34; and in which, consequently, the length of the shadow at noon was sensibly the same as in *every preceding year*. This is a curious coincidence; and I should lay a good deal of stress on it, if I could depend on the inclination assigned to the pyramid by this writer being correct. It would appear from the measurement of the French engineers that it was more considerable; and that of the second pyramid, of which the top is in a finished state, is certainly so. It is not to be supposed that all the pyramids in a group were constructed for being used as chronometers. A single one would alone be required for this purpose, and the second pyramid at Jizeh was probably the one used. This would cast a shadow on the first day of the year, reaching to a perpendicular wall of rock, parallel to the north side of the pyramid. On this wall there are said to be hieroglyphics. It would be desirable that travellers in Egypt should ascertain if this be the case, and especially if the rocky wall be marked, opposite to the vertex of the pyramid, in any such manner as might constitute a sort of dial.

We must not suppose that the Egyptians waited for the construction of a pyramid in order to determine the commencement of their year, nor is it likely that there was any in existence so early. They used such a pointed object as they could readily procure; fixing upon some one object, and some one length of its shadow, as standards. The standard of measurement was easily *copied*. It was only necessary to have a suitable object previously prepared, and on the first day of the year, as indicated by the original standard, to mark the length of the shadow of that object. In this manner the standard of measurement might be transferred from one place to another; and at length such gigantic objects as the pyramids were every where used. But this led to an inconvenience, which was probably a principal cause of the wandering year being substituted for the fixed one. The indications of these standards would not always agree; and the longer the measure used, and therefore, apparently, the more accurate the measurement, the more distinguishable would be the dif-

ference of the indications. The cause of this diversity may be easily seen. At the end of 365 days, the altitude of the sun would not be precisely the same as at the beginning, but about 5′ greater, his altitude decreasing something more than 20′ a day. If the standard were copied on the first day of this next year, the length of the shadow would evidently be, as compared with the original standard, too small, in the proportion of the cotangent of the altitude, increased by these five minutes, to the cotangent of the altitude itself. The consequence would obviously be, that the new standard would indicate a different series of years of 366 days from the original standard; it would point out the same first of Thoth in three out of four years, but one a day earlier in the fourth. To obviate this error, it would probably be soon agreed, that the standards should only be copied at the end of the years of 366 days, when the altitude would be nearly the same as at first. But this would only diminish the evil. In the first place, it could not in every instance be certainly known before-hand, whether the current year would consist of 366 days; it might be a matter of doubt, until the shadow decided the question, whether such a protracted year would, on this particular occasion, occur at the end of three or of four common years. But, setting aside this consideration, the copy taken at the end of a year of 366 days could never *perfectly* represent the original standard. The altitude at the end of four years would be somewhat different from what it was at the beginning; and though the copy then taken would agree with its original in indicating the *first* few years of 366 days, it would before long indicate different ones, its quinquennial periods being interspersed among the quadrennial ones in a different manner.

Under these circumstances, we cannot wonder that the Egyptians should consider the existence of these years of 366 days as a nuisance, and should in course of time determine to get rid of it. Had they occurred at settled intervals, they might have been tolerated; but, occurring as they did irregularly, and the standards in different parts of the country indicating different times for their occurrence, they would be a constant source of annoyance and contention. It was at length resolved that there should be no more intercalation, but that the twelve months and the five celestial days should constitute the entire of the year. The period when this change took place is indicated by the names of the months, hieroglyphically given to those of the year when a fixed one, and

retained through the influence of custom after it had become a wandering one. Those names indicate physical characters, which the months of the wandering year could only have between the limits 1800 and 1760 before our era. Within these limits the reformation of the calendar must have taken place; and it will be the object of the following researches to establish the precise year, in which the new system was introduced.

III. I have already intimated that I have been directed in this inquiry by a passage in Tacitus. It is the twenty-eighth chapter of the sixth book of his Annals; and before I go further, I shall give a translation of the material part of this chapter.

"In the consulship of Paullus Fabius and Lucius Vitellius, after a long course of ages a phœnix arrived in Egypt, and caused much conversation respecting it among the most learned, both of the natives and of the Greeks. I will state those facts, about which there is an agreement, as well as some others that are doubtful, but not undeserving of being known. Those who have described its appearance are agreed that it is consecrated to the sun, and in face and plumage unlike to other birds. Different accounts are given respecting the number of years that it lives. The most common statement is 500 years. Some say that the interval is 1461 years; and that former birds flew into the city of Heliopolis (attended by great numbers of other fowls, which were astonished at the strange appearance) first in the reign of Sesostris, afterwards in that of Amasis, and next in the reign of Ptolemy, the third Macedonian sovereign (Ptolemæo qui ex Macedonibus tertius regnavit). But the chronology is certainly obscure. Between Ptolemy and Tiberius were less than 250 years. On this account, some have supposed that this last was not a real phœnix; that it did not come from the land of Arabia, nor do any of those things which the old tradition has recorded." Then, after describing the manner in which the phœnix provides itself with a successor, he concludes: "These things are uncertain, and in part fabulous; but there is no doubt that this bird is sometimes seen in Egypt."*

* Paullo Fabio, L. Vitellio Coss. post longum sæculorum ambitum, avis phœnix in Ægyptum venit, præbuitque materiem doctissimis indigenarum et Græcorum, multa super eo miraculo disserendi: de quibus congruunt, et plura ambigua, sed cognitu non absurda, promere libet. Sacrum soli id animal, et ore ac distinctu pinnarum a ceteris avibus diversum, consentiunt qui formam ejus

It is evident from this last sentence, that Tacitus, and those from whom he derived his information, were completely mystified by the Egyptian priests; and that they supposed the phœnix to be a real bird. Pliny appears to have thought the same. He speaks of it in his Natural History B. 10, c. 2; and, while he mentions 660 years as the length of its life, he preserves an important statement of Manilius, that "in the life of this bird a revolution of the Great Year was completed, and the seasons and stars returned to the same situations." Brotier, in his note on the above cited passage in Tacitus, after correcting an absurd mistake of Hardouin, who understood Manilius to speak of the paschal cycle of 532 years, gives it as his own opinion, that he spoke of the canicular cycle of 1461 years; after describing which, he says, "This is that most celebrated revolution of the Great Year, and restitution of the zodiac, *which was shadowed forth by a bird, sacred to the sun, and renewing its existence from itself; whence the Egyptian fable of the Phœnix originated.*" There can be no doubt in the minds of any, who are acquainted with Egyptian literature, that this idea of Brotier's is a correct one, and that *the appearance of a phœnix* was a mystical mode of expressing *the renewing of a cycle.* He had, however, no right to assume that the cycle spoken of by Manilius was the canicular cycle, or that the Egyptians used no other cycle than this. We learn from Censorinus, that that cycle was renewed A. D. 138; the phœnix whose life was 1461 years appeared at that time; but we learn from this passage of Tacitus that some phœnix made its appearance A. D. 34, which was the year in which Fabius and Vitellius were consuls. We learn also that this phœnix, or a different one, had previously appeared in the reigns of Sesostris, of Amasis, and of Ptolemy Phila-

definiere. De numero annorum varia traduntur: maxime vulgatum quingentorum spatium: sunt, qui asseverent, mille quadringentos sexaginta unum interjici; prioresque alites, Sesostride primum, post Amaside dominantibus, dein Ptolemæo, qui ex Macedonibus tertius regnavit, in civitatem, cui Heliopolis nomen advolavisse, multo ceterarum volucrum comitatu, novam faciem mirantium. Sed antiquitas quidem obscura; inter Ptolemæum ac Tiberium minus ducenti quinquaginta anni fuerunt; unde nonnulli falsum hunc phœnicem, neque Arabum e terris credidere, nihilque usurpavisse ex his, quæ vetus memoria firmavit: confecto quippe annorum numero, ubi mors propinquet, suis in terris struere nidum, eique vim genitalem affundere, ex qua fœtum oriri; et primum adulto curam sepeliendi patris; neque id temere, sed sublato myrrhæ pondere, tentatoque per longum iter, ubi par oneri, par meatui sit, subire patrium corpus, inque solis aram perferre atque adolere. Hæc incerta et fabulosis aucta. Ceterum aspici aliquando in Ægypto eam volucrem non ambigitur.

delphus or Evergetes. The latter of the two was evidently the one of whom Tacitus was thinking, when he said that between Ptolemy and Tiberius there were less than 250 years; though this observation is incorrect, even in reference to Evergetes, unless we count from the end of his reign to the beginning of that of Tiberius. We should, however, recollect that Tacitus is here copying the words of some other writer, and that *he* may have considered Alexander as the first Macedonian sovereign of Egypt, though Tacitus overlooked him as such. The writer of the article in Fraser's Magazine conceives that apocatastatic cycles of 1460 years terminated at the several epochs, at which Tacitus places the appearance of a phœnix; and he thinks that *one* of those cycles commenced at the chronological epoch of the eighteenth century before Christ, and ended in the reign of Ptolemy Philadelphus. This might be readily admitted; but at what chronological epoch can we fix the commencement of that cycle, which terminated A. D. 34? or that, which terminated in the reign of Amasis; 1461 years before which, the year of 365 days was not in use, according to this gentleman's system, any more than according to that, which I have endeavoured to establish in opposition to it? Besides, Tacitus evidently intimates, by what he says of the interval between Ptolemy and Tiberius, that these appearances had been recorded by the author whom he follows, as a connected series, and not as a number of independent ones.

On these grounds, I concluded that a series of cycles, of some sort or other, must have terminated A. D. 34. The origin of them I could only fix at the reformation of the calendar in the eighteenth century before Christ; and what I had to do in order to ascertain their number, was merely, by comparing some one of the epochs mentioned by Tacitus with A. D. 34, to obtain such narrow limits for the length of the cycle, as that there could only be a single integral quotient, when this length should be made to divide the entire interval, which I had already restricted within the limits 1833 and 1793 years.

Of the three epochs which Tacitus mentions, the first was of no use to me, because even the age at which Sesostris lived is not among the *data* of chronology. Still less could the limits of his reign be so. The last was likewise insufficient for my purpose; for the possible limits, which it gives for the appearance of the phœnix, are 285 B. C., the beginning of the reign of Philadelphus, and 222 B. C., the end of the reign of Evergetes. The limits of the interval

are consequently 318 and 255 years, which will allow of being repeated six or seven times between the chronological epoch and A. D. 34. The penultimate appearance, in the reign of Amasis, was, consequently, that, on which I had to depend. Amasis reigned 44 years, down to about half a year before the Persian conquest. No chronologer has fixed this conquest later than 525; which is, therefore, the latest date, at which this phœnix could have appeared. There are cogent reasons, however, for placing it two years earlier; and I am inclined to think that the first year of Amasis was that which began in January, 572, B. C.*

* As this does not appear to be recognized as a truth by the students of Egyptian literature, and as the arguments in its favour can be very briefly stated, it will be well to state them here. 1st, Manetho, as quoted by Africanus, makes the reign of Cambyses over Egypt *six* years. 2nd, There is an inscription in existence near Cosseir, (Burton's Excerpta Hieroglyphica, pl. 8,) in which the duration of the Persian authority in Egypt, up to the period of its being cut, appears to be recorded. The period stated is *six* years of Cambyses, thirty-six of Darius, and twelve of Xerxes. 3rd, There is a pillar in the museum at Florence, the inscription on which reckons seventy-one years from the 3rd of Neco to the 35th of Amasis; whence it follows that Neco and his successors reigned thirty-nine years before the accession of Amasis. Now, the first year of Neco could not have been later than 610 B. C., as we know from Scripture that in that year he defeated Josiah. Consequently, the first year of Amasis could not have been later than 571 B. C. As the first year of Darius was 521 B. C., we have at least fifty years for the interval, viz. forty-four for the reign of Amasis, and six for that of Cambyses and Smerdis. It is probable, however, that the first year of Neco was 611 B. C., and that of Amasis 572 B. C. We may allow a year for the short reign of the son of Amasis; for the confusion attending on the conquest, and for the dominion of the Magi; and there will then remain forty-four years for Amasis to have reigned, and six for Cambyses.

With respect to the division of the intervening thirty-nine years, I believe Herodotus to be correct when he assigns sixteen years to Neco, and six to Psamitich II. Manetho, as we find his text in Syncellus's work, on the authority of Africanus, makes the two reigns to contain six years each. This, however, is an obvious mistake of a copyist as to the reign of Neco. There remain seventeen years for Apries or Uavre, "the priest of the sun," reckoning his reign to last from the death of Psamitich II. to the accession of Amasis. As to the latter limit, however, it is probable that Uavre lived some time after Amasis assumed the royal dignity, say two years. This would account for his reign having been reckoned as of nineteen years by Manetho; and it may have been reckoned by others as of twenty-five years, (the number given to him by Herodotus,) if his years were computed from the death of Neco. I suppose Psamitich II. and Uavre to have been brothers, and to have shared the sovereignty between them. I suppose, further, that the king, known to us from the monuments as Psamitich III., was the son of Psamitich II., and that in course of time he was deposed by his uncle. Amasis married the daughter of this prince, and avenged his cause by

I therefore take this as the earliest date. The least and greatest intervals are 558 and 605 years; halving which, I obtain 279 and 302½, as the least and greatest numbers of years that the cycle can contain. These limits are inconsistent with there being any number of cycles except *six* between the chronological epoch in the eighteenth century and A. D. 34. Five such cycles could not have exceeded 1512½ years, while seven could not have been less than 1953; but we have seen already that the entire interval could only vary about twenty years from its mean value 1813 years. It consequently became a matter of certainty that the number of cycles was *six;* and that the least possible value of each was 299 years, the sixth part of 1793. The greatest possible value has been previously determined to be 302½ years.

IV. The next point was to ascertain what period of time, having a cyclical character, and being such as the Egyptians would be likely to observe, was to be found within the narrow limits which have been now determined, 299 and 302½ years. I observe, in the first place, that *any* cycle, depending on a comparison of the wandering year of 365 days with a fixed year of any description, could not differ much from 1505 years, which is the nearest whole number to the quotient of 365 days by the excess of the mean tropical year over 365 days. It is evident then that the period which we wish to ascertain could not be any such cycle, *taken as a whole.* It might, however, be *a submultiple of it;* and it at once presents itself to us as a remarkable coincidence, that the only small divisor which 365 will admit, namely *five,* is the only integral quotient that could result from dividing a number which can but little differ from 1505 by a number lying between the limits 299 and 302½. We are not then driven to the necessity of seeking a lunisolar cycle, or a cycle connecting the revolution of the moon with the year of 365 days, which might lie between the prescribed limits; we at once

deposing Uavre; which, we know, he did by the aid of Nebuchadnezzar, king of Babylon, to whom he was in subjection for a considerable part of his reign.

The above appears to me the most probable mode of solving the acknowledged difficulties respecting the succession of the Saitic kings. I do not propose it as absolutely certain; but I cannot but regard it as what should be considered *a settled point,* that the conquest of Egypt took place in 527 or 528 B. C. I am aware of the confident statements of Herodotus and Diodorus to the contrary; but these are in my judgment far outweighed by the threefold evidence that I have adduced against them.

see that the cycle required was that, in which *some annual phenomenon* advanced seventy-three complete days through the wandering year; that is to say, in which it traversed through a fifth part of it. Now what phenomenon was so likely to be chosen as that, which had marked the commencement of the old fixed year, and for observing which there existed every facility? The only other annual phenomena, which suggest themselves, as at all likely to have taken the place of this, are the heliacal rising of Sirius, and the solstice or commencement of the inundation. The former of these is, however, out of the question, because the corresponding cycle would contain only 1460 years; the fifth part of which, 292, is less than the minor limit already ascertained. In fact, if we counted back six periods of 292 years from A. D. 34, we should reach the year 1719 B. C., when the physical characters of the seasons would by no means correspond with accuracy to those indicated by their hieroglyphical names. The difficulty of observing the solstice correctly, on account of the very great altitude which the sun then has in Egypt, as well as on account of the small change which its altitude then undergoes on consecutive days, renders it exceedingly unlikely that that phenomenon should be chosen; and there would be no advantage gained by observing the commencement of the inundation, as there is a want of regularity in it. Taking all these circumstances into consideration, it cannot, I think, admit of a doubt that the interval between the introduction of the wandering year and A. D. 34 contained *six-fifths of a complete cycle*, in which the phenomenon, which had marked the commencement of the old fixed year, travelled through all the days of the wandering year.

V. It only remains that we should determine the length of this complete cycle. But here, methinks, some one will ask—"have you not yourself already stated it? Is not this the cycle obtained by comparing the year of 365 days with the mean tropical year, which you have already stated to be 1505 years?" I answer that the *mean tropical year* is a mathematical abstraction, which may be *calculated*, but which cannot be immediately *observed;* and that of the *many* tropical years which may be *observed* the inequality is so great, as sensibly to vary the length of the cycle formed by comparing them with a year of 365 days. There are two distinct causes for the tropical year, *as it must have been observed by the Egyptians*, differing from the *mean* tropical year; and both of these causes have the effect of lengthening the year, and consequently of shortening

the cycle. They do this to such an extent, that 300 years will be found to be the true length of the smaller cycle, and not 301, as would be the case if we used the *mean* tropical year. The first of these causes is the annual change undergone by the equation of the centre, proper to the point in the orbit where the sun is situated at the commencement of the year. The sun's perigee passed through that point in the orbit about 400 years before the chronological epoch of the eighteenth century before Christ; whence it is easy to see that for a long course of ages about that epoch the sun would at the end of a mean tropical year be behind his place at the beginning of it; as the annual change in the equation of the centre would always lengthen the year.* The other cause of the year being

* Let θ be the sun's longitude at the commencement of any year, reckoned from the mean equinox of that time, and not corrected for lunar or planetary perturbations. Let θ' be the sun's longitude, reckoned in like manner, at the end of any time t. The elliptic theory of the planets gives us the following equations, n expressing the mean motion in longitude during that time in reference to the mean equinox;

$$\theta = \varepsilon + 2e \sin(\varepsilon - \varpi) + \&c. \qquad (1)$$

$$\theta' = nt + \varepsilon + 2e' \sin(nt + \varepsilon - \varpi') + \&c. \qquad (2)$$

The remaining terms of these values, containing the second and higher powers of the eccentricity, may be disregarded; as it is evident they can only modify in a very slight degree the results obtained from considering the two first terms. At the end of a tropical year

$$\theta' - \theta = 2\pi; \qquad (3)$$

and the value of t which satisfies this equation is, of course, the length of the tropical year. What is called the *mean* tropical year is the value of t, obtained by leaving out of consideration the part of the orbit in which the sun was situated at the beginning of the year; or, in other words, by considering only the *first* terms in the above values, which are independent of the angle $\varepsilon - \varpi$. In the mean tropical year, $\theta' - \theta = nt$; and therefore, by (3)

$$nt = 2\pi; \text{ or } t = \frac{2\pi}{n}. \qquad (4)$$

It is evident that this value of t would also satisfy (3), taking into consideration the other terms in the values of θ and θ'; provided only that e and ϖ were invariable. The divergency, then, of the various tropical years that may be observed from the mean tropical year is due to the secular variations of these elements. We know that e is continually diminishing, while ϖ is continually increasing. Let $e - \delta e$ and $\varpi + \delta\varpi$ express the values e' and ϖ', belonging to the end of the year; and let δt be the variation of the length of the tropical year, caused by the variations of the elements. It will obviously be a function of δe, $\delta\varpi$, and of the angle $\varepsilon - \varpi$; and it will depend on the magnitude of this angle (that is, on the part of the orbit where the sun is situated at the commencement of the year) whether it is to be added to the mean tropical year, or subtracted from it.

Substituting in (2) their values for e' and ϖ', and writing $t + \delta t$ for t; confining ourselves also

lengthened is the diminution of the obliquity of the ecliptic. The phenomenon, by which the commencement of the year was indicated, was the attainment of a given length by the meridian shadow of an object; that is, the diminution of the sun's altitude beyond a given limit, or his attaining a given south declination. Now, the sun being at this time in the quadrant following the equinox, he must not only attain the same longitude as he had at the beginning of the year before this can happen, but he must go over a small additional arc sufficient to compensate for the decrease of obliquity. The time of his passing over this small arc must be added to the mean tropical year, as well as the time of his passing over the annual variation of the equation of the centre; and the sum of all three will be the tropical year, as it would have been observed by the Egyptians. I do not mean to say that it would be precisely so in a single year. The lunar and planetary perturbations might make it greater or less. But, taking the sum of a few observed years, the effect of these perturbations would disappear, and the average value of the observed year would be that which I have stated.

It remains that I should ascertain the numerical value of this tropical year. I find, in the first place, that the annual precession, about the time of the chro-

to the first powers of the variations, as well as of the eccentricity, we have by equation (3)

$$0 = n\delta t\{1 + 2e\cos(\varepsilon - \varpi)\} - 2\delta e \sin(\varepsilon - \varpi) - 2e\delta\varpi\cos(\varepsilon - \varpi);$$

$$\delta t = \frac{2\delta e \sin(\varepsilon - \varpi) + 2e\delta\varpi\cos(\varepsilon - \varpi).}{n}$$

When the mean anomaly is less than 90°, both the terms, which compose the value of δt will be positive. It was so, in the case we are considering, from the earliest age that can be conceived to about the year 2170 B. C., when the perigee passed through the sun's place at the commencement of the Egyptian year. In the next quadrant, the term depending on δe is negative; but during the greater part of the time that the perigee takes to pass through it, δt will be positive, on account of the variation $\delta\varpi$ being greater than δe. If $\varepsilon - \varpi$ be greater than 180°, but less than 270°, δt will consist of two negative terms; and if it be less than 180°, but greater than 90°, its terms will be of opposite signs, but the negative one will preponderate.

In the interval between the chronological epochs of the eighteenth and third centuries before Christ, the average value of $\varepsilon - \varpi$ in reference to the summer solstice was about 218°. The tropical year commencing at that solstice was consequently less than the mean tropical year, both the terms of δt being negative; and of course the cycle, formed by comparing such a year with the year of 365 days, was greater than the cycle, formed by comparing the mean tropical year with the year of 365 days. The latter consisted of 1505 years; the former of 1508 years; the coincidence of the solstice with the 241st day of the year occurring in 1779 and 271 B. C.

nological epoch in the eighteenth century before Christ, was, by Laplace's formula, 49″,32; that is, 0″,78 less than the precession, with which Delambre's tables are calculated. I seek then in those tables the time in which the sun would describe 360°. 0′. 0″,78, and find it to be 365,242485 days, which I take for the length of the mean tropical year. I find the annual variation of the equation of the centre to be 2″,1125, taking into account both the decrease of the mean anomaly and that of the eccentricity. The time of describing this arc would be 0,000595 of a day. Lastly, the annual decrease of the obliquity of the ecliptic is 0″,4238. This must be compensated for by an increase in the longitude of 0″,58845; and the sun would take 0,000166 of a day to describe this arc. Adding together these three quantities, we have for the value of the tropical year, *as the Egyptians would observe it*, but independent of lunar and planetary perturbations, 365,243246 days. I now divide 73 days by the excess of the last number over 365, and the quotient is 300,1077 years.* When we consider that this is the value of the cycle, calculated on the supposition that the length of the year at the beginning of the eighteenth century had always been its length; but that, in point of fact, its length had been for many previous centuries constantly decreasing, it will be obvious that the Egyptians, looking to their past observations, could not possibly have estimated the lesser cycle at more than 300 years. I have built nothing on the consideration of this being a round number, though that is a circumstance that would not be likely to be overlooked, even had 301 been a somewhat more accurate cycle; but I contend that, accord-

* There can be little doubt that Laplace's formulas give the obliquity and its annual variation in past ages too great, and the precession too small. In the question respecting the heliacal rising of Sirius, the correction of this error would have been in my favour. Here it is the reverse; and, therefore, candour obliges me to notice it, and to estimate its bearing on the strength of my argument. The difference between the precession now and in 1780 B. C., as estimated by Laplace, cannot have exceeded its true value by so much as 0″.06. The excess was probably much less; but I am now taking extreme values. The annual decrease of the obliquity must have been at the least 0″.39. The difference between this and the value in the text would be compensated for by a difference of 0″.047 in longitude. The total difference is less than 0″.107; over which the sun would move in 0.00003 of a day. We should thus have 0.243216 for a divisor, in place of that in the text; which would give 300.144 for the quotient, determining the length of the cycle. The difference between this and the value given in the text is evidently immaterial, so far as our present argument is concerned.

ing to the principles laid down for computing the value of the tropical year, it is such as will lead to 300 and not 301 years, as the time in which the sun would descend to the standard altitude on the seventy-fourth day of a year made to consist always of 365 days.

Counting back six of these periods of 300 years from A. D. 34, I arrive at 1767 B. C., in which year the commencement of the wandering year was on the 8th November. On that day, therefore, the new system must have been adopted; and the first Egyptian year of 365 days must have been the one, of which that was the first day. The longitude of the sun on that day was about 211°. 39′; and its declination about 12°. 18′ south. We may therefore safely conclude that $12\frac{1}{4}°$ was nearly the standard declination; and that up to this epoch (1767 B. C.) *the first day in which the sun's south declination exceeded* $12\frac{1}{4}°$, *was the first day of the year*. I will only add, that the first day of the year, computed in this manner, will occur at the end of 300 years on the 109574th day from the introduction of the system; those 300 years containing 109573 days, or $300\frac{1}{5}$ Egyptian years of 365 days. And not only so, but this will continue to be the case for no less than ten periods of 300 years, or two complete revolutions of the seasons. I find that on the 19th October, A. D. 1234, which was 3002 years of 365 days from 8th November, 1767 B. C.; and which would have been the 1st Thoth of 3003rd Egyptian year, had such continued in use; the sun's declination was less than 12°. 35′; and consequently this was the first day of its exceeding 12°. 15′; for the diurnal increase of the declination was, at that time, and in that part of the orbit, near 21′. If we trace the period backward, in place of forward, its accuracy is considerably greater. In the 900 years preceding 1767 B. C. the change of declination would not amount to a minute; and in the preceding ages, if we choose to calculate what would have occurred before the colonization of Egypt, the cycle would be so exact, that the change of declination in 300 years would be scarcely observable.

VI. I now proceed to mention some important verifications of these results, which I have obtained from independent considerations, since I first arrived at them. These verifications respect first *the length of the cycle*, and secondly *the date of its epochs*.

1. It is a very remarkable circumstance, that the double period of 300 years, or the time in which the attainment of $12\frac{1}{4}°$ south declination by the sun would pass from the first to the 147th day of the wandering year, is a lunisolar cycle of

singular accuracy, being nearly equal to 7421 synodical revolutions of the moon. In fact, if we assume 600 tropical years to be equal to 219146 days; which in those remote ages was scarcely more than their true value, according to the Egyptian mode of observation; and if we equate this value to 7421 lunations; we obtain for the length of each lunation 29,53052 days. The true length of a lunation was in the 18th century before Christ about 29,53060 days; the error being only one 12500th part of a day in each lunation, or, more accurately, 5916 ten thousandth parts of a day in 600 years. There can be no doubt that the lunisolar cycle of 600 years was observed in the most remote antiquity.* The knowledge of it, acquired in antediluvian times, would be preserved by the survivors of the deluge; and would be carried at the subsequent dispersion to all countries. The Egyptians would thus have been led to pay particular attention to this period of 600 years; and, when they noticed the remarkable fact, that in the half of that period there occurred just 73 years with 366 days; and, consequently, that it would be equal to $300\frac{1}{5}$ years of 365 days; they would be the more easily persuaded to adopt a wandering year, which connected itself so readily with the cycle that was already in use among them. I here speak of the men of learning among them; for, as to the bulk of the population, all *their* feelings would be in favour of a year of uniform length, and in opposition to the nuisance of the 366th day, which, occurring so irregularly as it did, was a constant source of vexation to them. The period of 300 years was thus the half of the lunisolar cycle of 600 years, and at the same time the fifth part of the cycle of 1500 years, which commenced at the time that the year began to wander, and would terminate when the 1st of Thoth returned to its original place in respect to the seasons; when the sun would again attain to $12\frac{1}{4}°$ south declination at its noon. Properly speaking, the period of 300 years was not *cyclical.* At the end of it, nothing returned to the situation in which it was at the beginning of it; but it was the greatest common measure of the two cycles which the Egyptians used, and consisted of an integral number of days; it thus possessed two characters, which entitled it to especial notice. Another period, also claiming especial notice, was the great period of 3000 years, which was the least common multiple

* It has been often remarked, that Josephus speaks of the cycle of 600 years as having been known to the antediluvians; accounting for their discovery of it by the great lengths of their lives. —See Ant. Jud. I. 3. 9.

of the two cycles; containing five lunisolar cycles, and two cycles of 1500 years. This period of 3000 years is mentioned by Herodotus (Eut. 123) as that in which the transmigration of souls is completed; and it appears from the Old Chronicle to have been the duration of the reign of Chronus, or Time.

But an objection may here be stated. If the period of 300 years was not in strict propriety cyclical, why should the Egyptians have represented it by a phœnix, which could only symbolize a period, in which things returned to their pristine state? This objection admits the following answer; which, if it be correct, supplies a fresh verification of the results, at which I have already arrived. The Egyptians did not place the return of a phœnix at the end of *every* period of 300 years; but only when the multiplier was an even number or five. In other words, using their mystical language, no phœnix lived so short a time as 300 years; but as one phœnix lived 600 years, and the other 1500, the intervals between the successive appearances of phœnixes would *sometimes* be only 300 years. Now, it is to be observed, that, though the period of 300 years, which terminated A. D. 34, was the sixth such period, since the reformation of the calendar in 1767 B. C.; the phœnix which appeared then is only numbered by Tacitus as the *fourth*. Why? because at the end of the *first* and *third* periods of 300 years, there was no *complete revolution*, and consequently no *phœnix*. Tacitus's phœnixes appeared first under Sesostris. This was the lunisolar phœnix, whose life was 600 years, which is the space mentioned by Philostratus in the third book of his life of Apollonius. The time of its appearance was the 147th day of the 601st Egyptian year, or 4th November, 1167, B. C. The next phœnix, which Tacitus mentions, was of the same sort; and appeared under Amasis on the 293rd day of the 1201st Egyptian year, or 31st October, 567 B. C. This falls within the reign of Amasis according to any system of chronology; and, according to what I conceive to be the most probable system, it falls in his sixth year. The third phœnix of Tacitus was that of which Manilius speaks; saying, that "in its life a revolution of the great year was completed, and the seasons and stars returned to the same situations." Its life was accordingly 1500 years, and it returned, *alter et idem*, on the first day of the 1502nd Egyptian year, or on the 29th October, 267 B. C.; which was the 19th year of Ptolemy Philadelphus, the third Macedonian sovereign of Egypt. Lastly, the lunisolar phœnix appeared under Tiberius, on the 74th day of the 1802nd Egyptian year, or 27th October, A. D. 34. We have thus all the phœnixes, whose

appearances are recorded by Tacitus, accounted for; and the only scruple, which any one can have, in respect to the coincidences of these appearances with the reigns of the kings whom Tacitus mentions, is whether Sesostris reigned at so late a date as 1167 B. C. I will not discuss this point. I will only observe that, according to Mr. Cullimore, who has paid much attention to Egyptian chronology, the construction of "the Memnonium," as the palace of this sovereign at Thebes has been most improperly called, took place about 1138 B. C.; and he reigned sixty-eight years, according to Manetho;* so that, if Mr. Cullimore be right, he might well have commenced his reign before 1167. I take it for granted, that we are to understand by Sesostris, Rameses the Great; the second sovereign of that name, whose numerous monuments exhibit him as the most distinguished of a race of conquerors.

2. I now come to some verifications of the epochs, that I have assigned for the commencements of these cycles. Connected as they are with one another, it is evident, that, if any *one* can be verified, independently of the series, the verification will extend to *all*. I at first considered the statement of Tacitus as so explicit with respect to the year 34 being that of the appearance of the phœnix, that I felt little desire for any verification of it, so soon as I became quite satisfied that the cycles of 600 and 1500 years were established on sure grounds. I was, however, startled at finding that the appearance of this phœnix was fixed by Pliny in a different year. He speaks of it (B. 10, ch. 2) as having arrived in Egypt in the consulship of Papinius and Plautius, that is, in A. D. 36. From the whole train of argument that I have used, it is evident that this change in the year of appearance of the phœnix, supposing it to be established, would only alter the epochs of my cycles, bringing them down two years, or 730 days;

* There is a tablet in the British Museum, dated on the 29th of the first month of the Inundation, (Pachon,) in the sixty-second year of this prince. Mr. Cullimore fixes the date of the Memnonium from the astronomical sculptures on its ceiling. It is but fair, however, to state, that from the very same data M. Biot places the building of this edifice in 1500 B. C., and Mr. Wilkinson in 1322 B. C. Though I feel disposed to agree with Mr. Cullimore, I by no means regard his system as established on perfectly sure grounds; nor do I consider my own conclusions, contained in this paper, to be so connected with it, as that they would be shaken by its being overturned. Tacitus may very well be supposed to have used the name "Sesostris" *indeterminately;* not for the great Rameses, but for some of the numerous princes who claimed descent from him, and bore his name. All, I presume, will admit that the sovereign who reigned in 1167 B. C. was a Rameses; and Tacitus may have meant nothing more than this.

making, for example, the origin of the system to be the 7th of November, 1765 B. C., in place of the 8th November, 1767. Being desirous, however, of getting rid even of this small uncertainty, I considered, in the first place, whether Tacitus or Pliny was *a priori* most worthy of credit, as to his date of this phenomenon; and, in the second place, what confirmation there might be obtained of either date from independent sources.

As to the first point, it appears to me that an annalist, like Tacitus, recording the events that occurred in the period of which he treated in their regular order, would be much less likely to go astray than a writer, like Pliny, who merely recorded the date of an isolated fact. The confusion, which existed among the chronologers of that period, with respect to the correct epoch of the building of the city, might easily lead to an error of two years; for, though Pliny describes the year, in which the phœnix appeared, by its consuls, and not by the year of the city, the author from whom Pliny copied may have used this latter mode of describing the year; and Pliny may have used consular Fasti, constructed on a different system from those which his author used; e. g. the latter may have stated, that the phœnix appeared A. U. C. 787, which would coincide with the consulship of Fabius and Vitellius, according to the chronology of Varro, or with A. D. 34; but Pliny may have understood him as speaking according to the chronological system of Cato, in which A. U. C. 787 coincides with the consulship of Papinius and Plautius, or A. D. 36.* Now, that this is the true mode of accounting for the difference between Tacitus and Pliny, and that the former was consequently in the right, is, I think, clearly established by this fact. In the same passage, Pliny gives another consular date, which is likewise two years after the date which would have been correct. Speaking of the canicular cycle, he says that the 1225th year of it (for that is evidently what we should read; the M standing for 1000 having been dropped by a careless transcriber) coincided with the year in which P. Licinius and Cn. Cornelius were consuls. Now, the year of their consulship began in October or November of the proleptic Julian year 98 B. C.; but the canicular cycle was renewed A. D. 138, according to the express testimony of Censorinus. The year which began in July, A. D. 138, was the first of the new, or the 1462nd of the old cycle; whence it is easy to see, that the year of that cycle, which would begin in September, 98 B. C., must

* Niebuhr, in the thirty-seventh chapter of the second volume of his History of Rome, points out an error of Livy of the same magnitude as this, which he attributes to a similar cause.

be the 1227th year. The 1225th would begin two years earlier, coinciding with the consulship of Antonius and Posthumius.

This might be considered as decisive in favour of Tacitus having assigned the correct year of the arrival of the phœnix; but a confirmation from another source is certainly desirable. I find this confirmation in the chronological system of the Persians, who had also a wandering year of 365 days, and who had also periods of 600 years, of which one commenced in 1767 B. C., the very year that I have assigned for the commencement of the Egyptian cycles. That the Persian cycle began in 1767 B. C. I establish in the following manner. The era used by the later Persians was that of Yezdegird, which dates from 16th June, 632 A. D. Yezdegird appears to have reformed the Persian calendar by adding an intercalary month at the end of every 120 years; before his time the year was a wandering one, like that of the Egyptians. Now it is said that the first year of Yezdegird was the 2401st year of the old era, *supposed* to be that of Jamshid; and, if we count back 2400 years of 365 days from 16th June, 632 A. D., they will lead us to 6th February, 1767 B. C. It is, however, inconsistent with what is expressly stated on the subject, that this old Persian era should be the era of Jamshid. His era, we are told, began at the vernal equinox. We must therefore go back about 240 years to 2007 B. C., when the equinox and the Persian new year coincided on the proleptic 7th April, in order to reach the era of Jamshid. Of what then did the epoch take place in 1767? To this question I can only give one answer; and, when we couple it with the fact that the Egyptian cycles began in the same year, it must be admitted to be highly probable that the answer is a correct one. The answer is this. *In the year* 1767 *B. C. the old lunisolar cycle of* 600 *years came to an end.* The Egyptians, who were desirous of substituting the wandering year of 365 days for their ancient year, of the inconveniences of which they were long sensible, thought the arrival of this epoch a good opportunity for making the change; and the first year of the new cycle of 600 years was their first wandering year, and consequently the first year of their cycle of 1500 years. The Persians had adopted the year of 365 days 240 years before; but, on the arrival of this epoch, they counted their years from *it*, rather than from the introduction of the wandering year by Jamshid; and, what is very remarkable, they appeared to have retained the use of cycles, or rather *systems*, of 600 years, after they had totally abandoned the use of the tropical year, and when these periods had consequently lost, or at

least changed, their cyclical character in relation to the revolutions of the moon. I say "changed;" for I am aware, that the period of 600 years of 365 days had a cyclical character, as well as that of 600 tropical years. The difference between these two was, as we have seen, 146 days, which is nearly equal to five lunations; and thus, while 600 tropical years nearly equalled 7421 lunations, 600 Egyptian or Persian years nearly equalled 7416 lunations. The latter two numbers being each divisible by 24, we have 25 Egyptian years nearly equal to 309 lunations; a cycle, which was, of course, well known to both Egyptians and Persians, when they had been any length of time using the wandering year. It is a remarkable result of what has been now ascertained that the years of the Egyptians and of the Persians were connected in a uniform manner; and that, reckoning them from their respective epochs, there could never be above a year difference in the date. The first day of the Egyptian year was the 276th of the Persian year that bore the same number; while the first day of the Persian year was the 91st day of the Egyptian year, which was numbered one less.*

But it occurred to me, that, if what I have stated be correct, there would be an important verification attainable of the fact, that the year which began A. D. 1767 was the first of a lunisolar cycle. It must have the astronomical characters of such a year. These characters are not to be sought in the Persian year, which began 6th February, 1767, nor yet in the Egyptian year, which began 8th November, 1767; but evidently in the old year, such as existed in antediluvian ages. This year is generally admitted to have begun at the autumnal equinox. Now the point to be ascertained was this. Was the new moon, which occurred

* I have derived my information respecting the Persian year from a comparison of what is stated in the Encyclopedia Metropolitana; Art. Calendar (where the authority quoted is Playfair's Chronology) with Barret on the Zodiac, p. 7, who quotes Fréret. I have endeavoured to separate the facts stated by these writers on ancient authority from their own inferences from them. The former I have retained; the latter I have criticised, and in general rejected.

The historic facts mentioned by these authors are the three following, viz. that the era of Yezdegird commenced on the 16th June, A. D. 632; that the first year of it was the 961st of a period of 1440 years, which had been preceded by another similar period; and that after the time of Yezdegird a month was to be intercalated at the end of every 120 years. The nature of the year before Yezdegird is matter of hypothesis. Fréret supposed it to have had intercalary months in the same manner as afterwards. I cannot but regard this opinion as unfounded. If Yezdegird made no change in the form of the year, there appears no reason for the Persians dating their years from his reign. He was an unfortunate prince, with whom his dynasty ended.

nearest to the autumnal equinox in 1767 B. C., at such a distance from the equinox as would naturally result from the incorrectness of former cycles? On examining into this matter, I found that it was precisely so. In 1767 B. C. the autumnal equinox occurred on the 8th October, and the new moon four days after, on the 12th. Now the new moon gained on the equinox about a day in each cycle of 600 years; consequently, in 3567 B. C., three cycles back, the new moon would occur the day after the equinox; and, taking into account the uncertainty of observation, at that early age, we might very well place the commencement of the series of cycles at that epoch. Those, however, who, with the early Christians, and most of the learned in modern times, adopt the chronology of the Septuagint version of the Scriptures, will naturally place the commencement of this series of cycles at the autumnal equinox 4167 B. C., when the coincidence of the equinox with the new moon was still more exact.

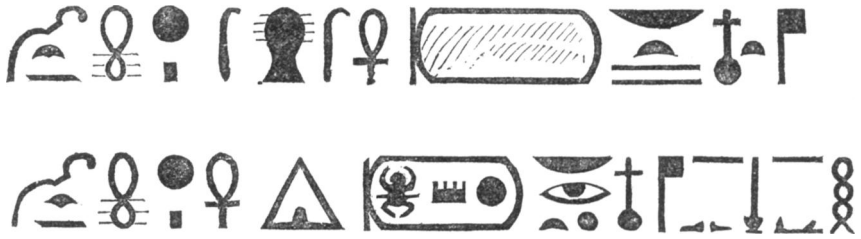
There is only one other subject, connected with the Egyptian year, on which I feel it necessary to make any remarks; and that is the canicular cycle. I conceive that what I have said has completely overthrown the theory of Fréret, followed by many in our own day, that two such cycles had elapsed in 138 A. D.; or that the first year belonging to these cycles began in July 2783 B. C. It is *possible*, that in 1323 B. C., the heliacal rising of Sirius being observed on the first Thoth, a cycle may have been adopted, grounded upon the occurrence of this phenomenon. But it seems much more probable, that some astronomer of the Alexandrian school, under the Ptolemies, observing the day on which Sirius then rose heliacally, and observing the rate of the progress of its rising (a day in four years) calculated at what time the rising would take place on the 1st of Thoth; and commenced the cycle proleptically at that time. In either case, we must suppose the Menophres, from whose time the years of this cycle are dated by Theon, to have been an Egyptian king; and there is no name to be found in Manetho, so likely to have been he, as Mephres of the eighteenth dynasty; who was probably the last Thothmos but one (the third or fourth); he whose sister shared the government with him in the beginning of his reign.* The interval

* In identifying this Thothmos with Mephres, (though not with Menophres,) I follow Champollion and Rosellini, who, however, makes him the *fourth* Thothmos, supposing there to have been in all five sovereigns of that name. I feel reluctant to differ from Mr. Wilkinson on this subject; but the evidence on which the identification rests is such as to carry conviction to my mind. This

between him and Rameses the Great, indicated by the monumental series, is not inconsistent with the supposition that the former reigned in 1323, and the latter in 1167.

Thothmos, we know, assumed the title Mæ-Re or Mæ-Phre, "lover of the sun;" for it appears in several of his shields; and he was the third in genealogical ascent from the "Memnon" of the vocal colossus, as Manetho makes Mephres to be. The father of Amenophis Memnon was, according to Manetho, Thothmosis; and the father of the Amenoph, of whom the colossus is a statue, was a Thothmos. Lastly, the son of Amenophis Memnon is called Horus by Manetho; and the son of this Amenoph has for his hieroglyphical name Amun-men Har-em-heb, "Horus in a panegyry;" —a coincidence of the most striking description. It is satisfactory to find the latter part of this name written *without abbreviation* in the twelfth plate of Mr. Wilkinson's Ancient Egyptians, as the name of the royal scribe who presides at the feast. The name is analogous to Muth-em-vaa, Har-em-vaa, Phtha-em-vaa, &c., i. e. "Muth in a barge," &c., which are common Egyptian names. But, to return to our Thothmos.—The only objection, that I am aware of, to his being Mephres, is the length of his reign. Manetho makes Mephres to have only reigned twelve years and nine months; but the thirty-fourth year of Thothmos is mentioned on the monuments. I answer this objection as follows:—Manetho, in giving this short reign to Thothmos, limits it to the time, during which he reigned alone after the death or deposition of his sister; but Thothmos dated the year of his reign from the period when he ascended the throne in conjunction with her, though, probably from his youth, with only nominal sovereignty. If his sister were the queen who erected the obelisks at Karnak, as I presume she was, we know that she took the credit of them entirely to herself, and the subsequent erasure of her name by Thothmos is a proof that there was little friendly feeling between the joint sovereigns, and affords ground for suspecting that the partnership in the crown, such as it was, was put an end to with violence.

But that Thothmos was really king in conjunction with his sister, and of course that he would count her reign as a part of his own, is proved by a statue in the British Museum, the inscription on which commemorates the reigning sovereigns as



that is, "the good goddess, the lady of the worlds, (defaced) may she live and be established like the Sun for ever! and her brother, the good god, lord of 'Achth,' (the Sun establishing the world, i. e. Thothmos III.,) may he live like the Sun for ever!" In the case of a single sovereign, we find the

I will now conclude this paper, which has much exceeded the limits that I at first contemplated. I entertain a confident hope that, if attentively considered, it will be found to establish some important points of chronology, and to establish them in perfect consistency with divine revelation.

title "Lord of the Worlds," prefixed to the first shield, while the title "Lord of Achth" precedes the shield which contains the phonetic name. I am not satisfied as to the meaning of this title; but the name Νεφαχθος, given by Diodorus to the father of Bocchoris, seems to be the expression of it in Greek characters.

In Manetho's list of the eighteenth dynasty, as handed down to us by Josephus and others, the name of Mephres immediately succeeds that of Amessis, i. e. Amenset, the sister of the first Amenoph. Rosellini supposes the sister of Amenoph to be the queen of the Karnak obelisks, and makes Mephres her son. Not having access to his work, I cannot say on what evidence he assumes this relationship to have existed between that queen and Mephres; but it is certain that Thothmos III. was brother, and not son, to the queen of the Museum statue; so that, if he were son to the queen of the Karnak obelisks, there must have been two queens regnant, his mother and his sister. However this may be, I feel quite satisfied that Queen Amenset, the sister of Amenoph I., was a distinct person from the queen who erected the Karnak obelisks.—I cannot venture to write down her name. I am inclined to think that three names are wanting in our copies of Manetho's list of sovereigns, answering to Thothmos I., Thothmos II., and Queen Amen—(?). We know that there is a deficiency of sixty years in *some* part of Manetho's list; for the total duration of the dynasty is distinctly stated by Josephus, more than once, to have been 393 years; while the sum of the reigns in the present copies of his list is only 333 years. Three reigns might well comprehend the sixty years that are deficient; and a copyist, having before him two queens' names *beginning alike*, might by an easy mistake place after the former of them the successor of the latter, omitting the second queen and the intervening kings. As for Mr. Wilkinson's hypothesis respecting Thothmos III., I consider it to be completely overturned by the fact, of which he does not appear to be aware, that his sister shared the government with him, or rather held it almost exclusively, in the early part of his reign. It has been also refuted on independent grounds, in a very satisfactory manner, by the author of the papers on the Pyramids in Fraser's Magazine.

The instances in which the Egyptian throne was filled by joint sovereigns, are already known to be pretty numerous; and further researches will doubtless augment their number. Amenoph III. had a brother, who for a time shared the government with him. Queen Taosre, who reigned in the interval between Rameses II. and Rameses III., had a brother, as well as a husband, for her partner in the throne. And I would suggest to the students of Egyptian literature, as well worthy of inquiry, whether the three sons of Rameses III. were not *joint sovereigns*; nay, whether all the other kings of that name, whose tombs are in the valley of the kings at Thebes, but of whose existence there seem to be no other monuments, were not the immediate descendants of these three kings, reigning cotemporaneously, and not in succession.

It may be well to annex the following series of chronological epochs, dated according to the principles which have been established in this paper :—

B. C.

3567. Oct. 20. A lunisolar cycle of 600 years commenced, the new moon and autumnal equinox coinciding.

2967. Oct. 16. Another similar cycle commenced.

2367. Oct. 12. Ditto. The knowledge of these cycles was preserved through the Deluge; and, subsequently to it, when Egypt was peopled, the years were reckoned according to this cycle; though the commencement was placed about a month after the equinox, when the Nile had fallen to a certain level, or rather when the sun had attained $12\frac{1}{4}^{\circ}$ south declination.

2007. April 7. Jamshid, king of Persia, introduced a wandering year of 365 days, which he made to commence at the vernal equinox, in place of the autumnal. The use of the cycle, of which this was the 361st year, was still retained.

1767. Feb. 6. The lunisolar cycle was renewed according to the Persian reckoning; and accordingly this was reckoned as the beginning of their *first* year. Their years were now arranged in periods of 120 and 1440, as well as of 600 years; and these were all years of 365 days.

1767. Nov. 8. The lunisolar cycle was renewed according to the Egyptian reckoning; and at the same time it was resolved that there should be no more intercalations; but that each year should consist of 365 days.

1323. July 20. Egyptian year 445, Thoth 1st; the first year of Menophres commenced.

1167. Nov. 4. Egyptian year 601, Choiac 27th; the lunisolar cycle was renewed. The phœnix of Sesostris appeared.

747. Feb. 26. Egyptian year 1021, Thoth 1st; the first year of Nabonassar commenced.

567. Oct. 31. Egyptian year 1201, Paoni 23rd; the lunisolar cycle was renewed. The phœnix of Amasis appeared.

B. C.

527. Jan. 2. Egyptian year 1241, Thoth 1st; the first year of Cambyses commenced.

332. Nov. 14. Egyptian year 1437, Thoth 1st; the first year of Alexander commenced.

267. Oct. 29. Egyptian year 1502, Thoth 1st; the cycle of the Seasons was renewed. The phœnix of Ptolemy appeared.

30. Aug. 31. Egyptian year 1739, Thoth 1st; the first year of Augustus commenced.

26. Aug. 30. Egyptian year 1743, Thoth 1st; the first day of the first fixed year; first used in Alexandria, and by degrees in all Egypt. Their 1st Thoth coincided with the 30th August in the years 26, 22, 18, &c. B. C.; but with the 29th in every other year.

A. D.

34. Oct. 27. Egyptian year 1802, Athur 14th; the lunisolar cycle was renewed. The phœnix of Tiberius appeared.

P. S.—I think it right to mention that, since the foregoing sheets were written, my views respecting the primary division of the Egyptian year have undergone a slight modification. I offered it as a not improbable conjecture, that a division of the year into three nearly equal seasons preceded the division into twelve months of thirty days each, with *epagomenæ* at the end. I am now quite satisfied, not only that this was the case, but that these seasons were subdivided into months containing alternately thirty and thirty-one days; the thirty-first day of the last month being dropped in the ordinary years; and that it was while this division subsisted, that the hieroglyphical notation of the months was adopted, and that the mythological connexion between them and the different deities was established. I was led to see this by considering the positions which the equinoxes and solstices would have in a year commencing on the first day that the Sun's south declination exceeded $12\frac{1}{4}°$. About the time of the reformation of the calendar in 1767 B. C. the vernal equinox would occur sometimes on the 150th and sometimes on the 151st days of such a year; while the summer solstice would fall sometimes on the 244th and sometimes on the 245th. But, if we go back a few centuries to the time when we may naturally suppose that

the names and the mythological patrons of the several months were assigned to them, the vernal equinox and the summer solstice would both fall a day later in the year. Now, it has been satisfactorily shown by M. Biot, that the normal position of the vernal equinox, according to his view of the matter, that is, according to mine, its position in the old fixed year, was in the fifth month, Tybi; while that of the summer solstice was in the ninth month, Pachon; and should have been at its very commencement. If we suppose, as I now do, that the months primarily consisted of thirty and thirty-one days alternately, both these conditions would be exactly complied with. The 151st and 152nd days of the year, between which the vernal equinox would fluctuate, would be the 29th and 30th days of Tybi; while the 245th and 246th days, on one or other of which the summer solstice would fall, would be the 1st and 2nd days of Pachon. On the other hypothesis of twelve equal months, and five or six epagomenæ at the end, the vernal equinox would fall out of the limits of Tybi; while the summer solstice would not occur before the 5th or 6th of Pachon. From these considerations it now appears to me quite evident, that the equalization of the months took place very little before the introduction of the wandering year, for which it prepared the way; if, indeed, the two changes did not take place at the same time; the five extra days of the alternate months being gathered together at the end of the year, on the occasion of the wandering year being substituted for the fixed one.